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# "The New Guinea Highlands" Region, Culture Area, or Fuzzy Set?

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# "The New Guinea Highlands"

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## Region, Culture Area, or Fuzzy Set?<sup>1</sup>

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by Terence E. Hays

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The criteria for delineating "the New Guinea Highlands," a fundamental category in Melanesian anthropology, are variable, vague, and inconsistently applied, with the result that there is little clarity or agreement with regard to its characteristics and its membership. So far as the literature is concerned, "the New Guinea Highlands" is a fuzzy set. The common resort to notions of "cores," "margins," or "fringes" is an attempt to preserve an essentialist approach but inevitably leads to the same confusion. The continued use of "the Highlands" as an analytic or theoretical construct carries the costs of misleadingly implied homogeneity, with marginalization of "exceptions," ahistorical reification of social and cultural "traits," and deemphasis on linkages among communities. A plea is made here for a shift from studies of morphology to studies of process—from concerns with what people are to concerns with what people do.

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1. This paper was originally prepared for a working seminar entitled "Not in Isolation: Regional Studies in Melanesian Anthropology," held in 1991 and cosponsored by the Wenner-Gren Foundation for Anthropological Research and the Field Museum of Natural History. I am grateful to those sponsors and the other participants in the seminar for a stimulating discussion of central issues and to Chris Gosden, Bruce Knauff, Paul Roscoe, Richard Scaglione, Robert Welsch, and the four referees for this journal for their very helpful comments on earlier drafts of the paper.

According to Mandeville (1980:549) "students agree on two things about New Guinea Highlanders: they exchange pigs and they do not conform to African models. A good start, but clearly more is needed." Whatever may be the limits of agreement among students, since the 1950s anthropologists have made a cottage industry out of demarcating a "region" called "the New Guinea Highlands" and trying to identify ways in which "the Highlands" can be contrasted with "the Lowlands" and what is to be found "there." For example, numerous differences in religion and cosmology have been proposed as points of contrast between "Highlands" and "Seaboard" societies (Lawrence and Meggitt 1965). More recently, Lindenbaum (1984:341) tells us that "from [a] larger, Melanesia-wide perspective, the New Guinea Highlands emerges as a region in which ritualized male homosexual experience is notably absent"—indeed, "the broadest contrasts among Melanesian cultures emerge . . . from a comparison between the so-called semen groups of the Lowlands and the Highland cultures in which semen is not the ritualized stuff of life" (p. 342). Whitehead (1986) contends that in "the lowlands" a "manhood emphasis" is to be found in fertility cults while in "the highlands" "clanhood" is emphasized. The list of characterizations and contrasts could be extended through social and political organization (e.g., Harrison 1989) to warfare (Knauff 1990).

These few examples are perhaps the kind of "more" that Mandeville feels is needed, and presumably they are the sort of claims that she has in mind in saying that "it makes more than geographical sense to think about the Highlands as a single area" (1980:550). In any event, they are indicative of how salient "the New Guinea Highlands" has become as a fundamental category in Melanesian anthropology as scholars have tried to develop explanations for social and cultural phenomena with reference to "regions" in which they do or do not occur or in which they take particular forms.

My concern here is to examine the category labeled "the New Guinea Highlands" as it has been used in several recent studies that offer explicit comparisons of "the Highlands" with other "regions" (Lindenbaum 1984, Whitehead 1986, Weiner 1988, Knauff 1990) or that incorporate major surveys of "Highlands" societies (Brown 1978, Gelber 1986, Feil 1987). These works are the result of literature surveys from which ethnographic cases have been drawn, categorized as "Highlands" or not, and compared for selected attributes. The criteria employed in these surveys and, consequently, their resulting classifications have varied considerably, and when their internal inconsistencies are combined with this variation in conceptualization the situation becomes even more muddled. We find ourselves in a position not only of wondering what we know after all about "the Highlands" but of questioning in what sense "the Highlands" is usefully regarded as a "region" at all.

## Delineating "the Highlands"

It might be supposed from its label that a category such as "the New Guinea Highlands" is basically organized around geographic or physical attributes, but which "lands" are "high" is not self-evident on an island whose relief extends from tide-washed coastline to snow-capped peaks at approximately 4,510 m above sea level in Papua New Guinea and about 4,740 m in Irian Jaya. Nor has there been agreement on the question among anthropologists.

In one of the earliest attempts to delineate the region, Read (1954:2) proposed that "the Highlands of New Guinea form a region which is . . . most simply described as a chain of valleys lying at heights of from four to seven thousand feet [1,212–2,121 m] and stretching roughly from east to west across the center of the country." The geographer Brookfield (1961:436) subsequently placed "the highland peoples" at "between lat. 3° and lat. 7° S., at altitudes ranging from 4300 to nearly 9000 feet [1,303–2,727 m] in valleys of the central cordillera." Focusing on the eastern half of the island, Bulmer and Bulmer (1964:39) extended "the Highlands of Australian New Guinea" to include "those parts of the Bismarck, Schrader and Central Ranges above 2,000 feet [606 m] which lie on the northern fringe of the [Eastern, Western, and Southern Highlands Administrative Districts]." For Brown (1978:13), "in highland valleys at an altitude of about 5,000 feet (1,520 m), and on the slopes above them, are the settlements and gardens of the highlanders. Between the mountain ranges surrounding these valleys and the New Guinea lowlands are steep slopes; the inhabited area lies between 3,000 feet (900 m) and 7,000 feet (2,100 m). This is the highlands margin and fringe." According to Gelber (1986:5), "[the] societies of the New Guinea Highlands . . . lie between 4500 and 8000 feet [1,364 and 2,424 m] in altitude," and Feil, while never explicitly demarcating the spatial boundaries of the subject of his recent book, provides a map (1987:38) labeled "peoples of highland Papua New Guinea" which highlights "land over 1200 metres." A final example of implicit thresholds may be adduced with Weiner's map (1988:4), which suggests 500–1,500 m as the "Southern Fringe Highlands Area."

Clearly, those who have tried to bound "the Highlands" geographically have taken seriously the implied salience of the adjective, but just as clearly they have adopted different cutoff points, with "high" apparently beginning as little as 500 or as much as 1,300 m above sea level.

Elevation per se appears not to be a sufficient criterion, however, since none of the writers whose works are considered here (nor any anthropologist of whom I am aware) routinely includes in "the Papua New Guinea Highlands" the high-elevation peoples of the Torricelli, Finisterre, and Owen Stanley Ranges (to cite only the most obvious candidates). Lack of contiguity with the central cordillera cannot account for all of the omissions. Geologically, the cordillera begins in the far west of Irian Jaya (a half of the island usually ignored or re-

ferred to very selectively [see Strathern 1990]), and in Papua New Guinea it stretches southeastward well into Milne Bay Province, including the country's third-highest mountain, Mount Victoria (at 4,072 m) in the Owen Stanley Range near Port Moresby (King and Ranck n.d. [1982]:88–89). Nevertheless, the Strickland Gorge on the west and the Kratke Range in the east are often the effective, if not explicitly stated, east-west boundaries of consideration. Such truncations cannot be understood as motivated by criteria based on relief or concomitant vegetation or climatic patterns (see King and Ranck n.d. [1982]:92–93, 96–97). Thus, Brown's (1978:2) claim that "altitude, climate, temperature, and other environmental characteristics set the highlands apart from the tropical lowlands" may be true for "the highlands" but it has not *in practice* been true for "the Highlands."

When explicit reasons are given for the exclusion of some high-elevation peoples, including some within the central cordillera itself, they tend to focus on subsistence types, staple crops, and population density. Most influential in this regard has been Brookfield's decision, in his review of the "distribution" of "the highland peoples of New Guinea" (1961:437), to drop the groups of "the Vogelkop to the west [in Irian Jaya] and the Kuku-kuku and Gailala areas to the east" because they "have not developed the intensive agricultural forms that are the best distinguishing characteristics of the highland peoples."<sup>2</sup> According to Brookfield (1964:21), an additional "characteristic of these Highlands people—one which distinguishes them from closely-settled populations at similar altitudes in other parts of the tropics—is their dependence on root crops, and especially on a single root crop, the sweet potato." Thus, using "a *sensu stricto* application . . . not merely the peoples on the outer slopes of the Cordillera, but also the inner montane Telefomin and Ok Sibil groups are excluded"; using "a *sensu lato* definition it is possible to include most of these other montane people, though *it becomes less easy to distinguish these* [emphasis added] from some of the adjacent lowlanders on the bases of agriculture or population density, and hence of ecological adjustment."

Brookfield must be credited with an earlier qualification (1962:252), proposing that the notion of a "simple region" must be "abandoned, and replaced by a series of cores showing gradations outward," but the suggestion is not a part of his main legacy in the literature. Thus, Brown (1978:4–5) states that "the most distinctive feature of highland culture is agricultural specialization, which supports large concentrations of people and periodic festivals at which thousands of visitors are entertained and feasted." For Lindenbaum (1984:343), "Highland societies are . . . based on the intensive production of sweet potato and domestic pig-herding, . . . whereas the smaller Lowland groups tend a different assemblage

2. In particular, Brookfield identified seven features of agricultural methods (dominant fallow cover, method of clearing, ground preparation, erosion control, water control, mulching and fertilization, and intercropping) as "the principal criteria for defining a 'central highland' region on the basis of agriculture" (1962:246).

of crops . . . accompanied by hunting and fishing." However, as Strathern (1990:379) has asserted, when one is examining "agricultural intensification, there is certainly no reason why cases from the Sepik, from Ok, from the highland fringes, or indeed from anywhere else, should not be chosen for discussion." Moreover, the distribution of sweet potato as the primary staple crop (King and Ranck n.d. [1982]:50–51) matches none of the current delineations of "the Highlands," and the same can be said for population distribution (King and Ranck n.d. [1982]:20–21).

While environmental, ecological, economic, or demographic attributes are the most common ostensible criteria for demarcating "the Highlands," in fact it is rare in the anthropological literature for such features to be privileged in explanations of, or even considered as being of much causal relevance to, the social or cultural traits that are the usual foci of attention. Instead, "big man" political leadership, ceremonial exchange systems, clan parish organization, bride price, and pig festivals recur so frequently in characterizations of "Highlands" societies that they almost achieve the status of diagnostic features. However, not only can one easily point to "Lowland" or island examples of such features but those who discuss them are often at pains to note and try to account for their variability *within* "the Highlands." It is precisely such variability (most systematically surveyed by Feil [1987] but acknowledged by nearly all of the scholars discussed here), as well as variation with respect to ecological and subsistence features, that has given rise to the increasing tendency to distinguish "core" from "margin," "fringe," and the like.

### Who Are "Highlanders"?

Despite Gelber's (1986:3) claim that "the Highlands have distinct geographical boundaries," the attributes most commonly used by anthropologists in defining or characterizing "the Highlands" as a regional category are variable, vague, and inconsistently applied. It is not surprising, then, that the ethnographic cases assigned membership in it differ as well.

Only rarely do the writers surveyed here list the cultural or linguistic groups included in the "Highlands" category. When they do, the lists are sometimes inconsistent with defining statements. Gelber, for example, says (1986:6) that "the Highlands" includes the groups "from the Enga in the west to the Fore in the east (the Mendi and Huli being the southernmost and the Maring the furthest north)" but then includes in her comparative table (pp. 10–11) the Tairora, who are in fact located east of the Fore. Most often, cases are simply adduced for illustrative or analytical purposes, and these are too variable to allow systematic comparisons of inferable lists of groups. This is understandable, perhaps, when surveys focused on a single topic (e.g., fertility cultism or ritualized homosexuality) must be guided by available information; failure to cite a given case in a presentation of "Highlands" forms, then, cannot necessarily be taken

as exclusion of that case from the "Highlands" category. In some instances, assignments seem idiosyncratic whatever the constraints imposed by the literature; for example, Knauff (1990:277) cites the Orokaiva and Tauade in his survey of "New Guinea Highlands warfare," and Whitehead (1986:87) includes the Awa and Ndumba (southern Tairora) of the Eastern Highlands Province along with such groups as Chambri, Iatmul, Abelam, and Arapesh in discussing the "manhood emphasis in the lowlands." In any case, some indication of the lack of consensus among anthropologists regarding the categorization of specific cases can be seen in explicit labeling when it occurs. This is most obvious with respect to the "Mountain Ok" groups, those of the "Bosavi region" and of the Karimui area, and the "Anga groups."

Craig (1990) has recently posed the question "Is the Mountain Ok culture a Sepik culture?" (for the prior question as to "the Sepik as a culture area," see Mead 1978, Brown 1991) and answered it "roughly" in the negative: "the societies most like the Mountain Ok societies are to the west, in the central ranges and foothills of the easternmost interior of Irian Jaya" (p. 129). Are they, then, "Highlanders"? For Feil, the answer is straightforward (1987:7, emphasis added): "Beyond [the "western highlands societies"], further to the west, are found societies of a different sort (for example those of Telefomin and other Ok groups), whose adaptation and cultural emphases are *unrelated to the highlands*." For others, their ambiguous status is made hardly less so by writers' phrasings. Thus Brown includes them on her map (1978:6) of "the highland area" but explicitly grants the Baktaman, Miyanmin, and Telefomin qualified membership as "fringe groups" (p. 13). Similarly, Whitehead (1986:86) considers "some Telefomin area groups" as "fringe" groups, yet later (pp. 89–95) cites Baktaman in her discussion of "clanhood emphasis in the highlands." So, too, Knauff (1990:280–81, emphasis added) locates Baktaman, Bimin, Miyanmin, and Ngalumin (with the latter referring to Atbalmin, not the Ngalum-speakers of Irian Jaya) "in the fringe areas . . . west of the Papua New Guinea highlands."

This employment of the qualifying adjective "fringe" is also characteristic, although not uniformly so, of treatment of the Bosavi area or "Great Papuan Plateau." Thus, while Brown included a chapter on the Etoro (Kelly 1976) in her coedited collection *Man and Woman in the New Guinea Highlands*, in her synthetic overview (1978:6) the Etoro and Kaluli are considered "fringe groups," as they also are by Gelber (1986:6), Whitehead (1986:86), and Knauff (1990:281) (the latter adding Bedamini, Gebusi, and Onabasulu from the same area). Feil is inconsistent, referring to the "Papuan Plateau" as "part of the congeries of peoples recently termed 'SWNG'—southwestern New Guinea coastal fringe" (1987:5–6, emphasis added) but including Etoro and Kaluli on his map (p. 38) of "peoples of highland Papua New Guinea" while excluding them from his table (pp. 42–43) showing "language family size in highland Papua New Guinea." Weiner (1988), in contrast to all of the others, rejects the Bosavi peoples as "fringe" groups, in-

cluding them instead in his new "Mountain Papuans" category.

The Karimui area is a bit more confusing, with Brown (1978:14) considering the Daribi a "Highlands fringe" group, Feil (1987:38) placing them ambiguously on his map but apparently considering them an "eastern highlands" society (p. 30), Knauff (1990:265-79) including the Polopa in his discussion of "New Guinea Highlands warfare," and Weiner (1988) counting both Daribi and Foraba (Polopa) as "Mountain Papuans." As for their ethnographers, D. J. J. Brown (1979:712, emphasis added) compares the Polopa to the Melpa, "*another* Highlands people," and Wagner both contrasts the Daribi with "Highlanders" (1967:11) and uses Daribi social organization (1974) to answer the question "Are there social groups in the New Guinea highlands?"

Finally, there are the "Anga groups," occupying highly diverse environments in the Eastern Highlands, Morobe, and Gulf Provinces.<sup>3</sup> Brown (1978:6) includes at least some of them on her map of "the highland area" but otherwise does not mention them in her survey of "highland peoples of New Guinea." The Baruya and the (pseudonymous) "Sambia" are consistently regarded as "fringe groups" by Lindenbaum (1984), Gelber (1986:6), Whitehead (1986:87 [adding Yagwoia as well]), and Knauff (1990:268). Again, Feil seems unable to make up his mind, referring to Baruya and "Sambia" as "living at the far eastern fringe of the highlands" (1987:176) and including them on his map but excluding them from his table.

We have seen, then, that in an arguably representative sample of recent anthropological writings on "the Highlands" no single attribute—environmental, ecological, demographic, social, cultural, or linguistic<sup>4</sup>—is a reliable predictor of which ethnographic cases will be included in the category either by a given writer or across

studies. Variability in inferable lists of "Highlands" societies follows as a matter of course.

### "Core" and "Fringe"

Clearly, much of the apparent diversity in delineations of the elevational boundaries of "the Highlands" results from the variable inclusion of the "fringe" or "margin." Thus, Read's proposal (1954:2), cited earlier, placed the valleys of the "central highlands" at 1,212-2,121 m, which is perhaps not significantly different from Brookfield's (1961:436) range of 1,303-2,727 m or Feil's (1987:38) "over 1200 metres." Brown (1978:13) seems to center them on 1,520 m, and this would be consistent with their placement on Weiner's (1988:4) map, where the upper limit of the "southern fringe" is 1,500 m. We might be justified, then, in the inference that "the core" of "the Highlands" is generally thought to be found at about 1,200 m and above, with the "fringe" extending down to 500 m in the south (Weiner) and 606 m in the north (Bulmer and Bulmer 1964:39).

However, as with "the Highlands" in general, elevation alone seems not to be *the* criterion for distinguishing "core" from "fringe"; indeed, some of the writers surveyed do not even seek to specify elevations. Brookfield (1962:253), as we have seen, appears to assess degree of "highlandness" in terms of intensity of agricultural techniques. Others have focused on other criteria. Weiner's (1988:3) are ostensibly geographical and ecological: "I refer to 'Fringe Highlanders' as those people who, like the Mountain Papuans, live in valleys on the edge of the central cordillera, valleys that are significantly lower in altitude and which consequently have a markedly different environment," with "the special features of the fringe dwellers" including "low population density, broad-based low-intensity subsistence production, and communal longhouse residence" (p. 2). For Knauff (1990:268), "peripheral and fringe areas of the New Guinea highlands" had "much lower population densities, ample land, and placed little if any emphasis on land acquisition through warfare." Gelber (1986:6) views "groups on the fringe areas of the Highlands" as differing "considerably from the Highlands in population density, horticultural practices, staple crop, reliance on hunting, comparative unimportance of pigs, and lack of elaborate exchange, as well as in ritual organization and in their sexual orientation and concerns." Whitehead complicates the picture somewhat by combining "fringe" groups with "lowlanders" in her analysis, as well as apparently extending her range beyond the central cordillera (1986:84, emphasis added): "On the margins of the highlands, and at middle elevations throughout the island, distinctly smaller population clusters practice mixed crop cultivation, modest (sometimes vanishingly modest) pig husbandry, and foraging. These groups, often termed 'fringe,' are quite varied in regard to ceremonial exchange." Feil (1987:5-6) proposes simply that the people of "the highlands of Papua New Guinea . . . have, for comparative purposes, been distinguished from the

3. It is not uncommon for authors of books for general audiences to use political boundaries in demarcating the "Highlands" region of Papua New Guinea. Thus, Sinclair (1971:ix) cites "the four Highlands districts—Eastern, Western and Southern Highlands and the Chimbu," just as Miller seems, judging from his map (1983:14-15), to employ the now-equivalent Eastern Highlands, Simbu, Western Highlands, Enga, and Southern Highlands provinces. None of the anthropologists whose works are examined here uses political boundaries in this straightforward manner. Indeed, it may be worth noting that among the peoples sometimes considered "highlanders" that are found *outside* of the five provinces listed above are the Kalam, Gende, and *some* Maring (Madang Province), all of the Mountain Ok (West Sepik and Western), Waffa and *most* Yagwoia (Morobe), Polopa and *some* Simbari (Gulf), and *some* Bogaya and *some* Duna (Western). Also, there are groups, usually not considered "highlanders," that straddle "highlands" and other provinces, among them the Hewa (Enga and East Sepik) and the Beami, Sonia, and Tomu River language-groups (Southern Highlands and Western provinces).

4. Feil's (1987:42-43) table is apparently based on Wurm's (1982) assignment of languages to the "East New Guinea Highlands Stock." A possible unstated linguistic bias elsewhere in the literature is suggested by the fact that among the Papua New Guinea groups that are *not* included in that stock are the Mountain Ok, the Anga, the Lake Kutubu peoples, those of the Bosavi area, and the Daribi and the Polopa—all of the groups most frequently considered either "fringe Highlanders" or "Mountain Papuans."

so-called 'highland fringe' groups and those of the Papuan Plateau . . . and even more so from the coastal 'seaboard' (particularly Sepik) peoples on the bases of geography, subsistence, language, and highly divergent aspects of society and culture," but he does not specify the nature of the distinctions between any two of these categories. Finally, for Brown (1978:13) "fringe" groups "seem mostly to be between lowlanders and highlanders in culture; many speak languages of groups also found in the lowlands. They are characterized by small and scattered settlements and partial dependence upon hunting and gathering."

In this array of characterizations of "fringe" peoples are common threads; in particular, "traits" such as smallness of populations and mixed subsistence economy seem to recur most frequently. Deferring to another occasion a systematic review of the ethnographic literature with respect to these and other variables, I would say here only that I find these traits difficult to consider as aptly describing the Telefomin (Brown 1978:13; Whitehead 1986:86; Knauff 1990:268), the Baruya and "Sambia" (Lindenbaum 1984; Gelber 1986:6; Whitehead 1986:86; Feil 1987:176; Knauff 1990:268), the Awa and southern Tairora (Whitehead 1986:87), and the Kewa, Wola, Maring, and Huli (Knauff 1990:268), to name only some of the purportedly "fringe" groups.

### "The New Guinea Highlands" as a Fuzzy Set

If the works reviewed here are representative of the current state of comparative studies in Melanesian anthropology (and I believe they are in many respects), it would seem that one of the fundamental categories used in such comparisons—"the New Guinea Highlands"—is employed with little consistency or clarity. However fundamental it may be to anthropological discourse and however much we may act as if it corresponded to a "real region," its use does not resemble that of what cognitive psychologists call "basic categories"—"information-rich bundles of perceptual and functional attributes . . . that form natural discontinuities" in the world (Rosch et al. 1976:385). Indeed, it does not even seem to be a category in the traditional sense of that term in set theory—a "logical bounded [entity], membership in which is defined by an item's possession of a simple set of criterial features, in which all instances possessing the criterial attributes have a full and equal degree of membership" (Rosch and Mervis 1975:573–74). Instead, "the New Guinea Highlands" as used in anthropological discourse exemplifies well what cognitive psychologists would call a "fuzzy set." In fuzzy-set theory (Zadeh 1965), "the referents of a word [or phrase] need not have common elements in order for the word to be understood and used in the normal functioning of language" (Rosch and Mervis 1975:574–75). Rather, "a family resemblance might be what [links] the various referents of a word. A family resemblance relationship consists of a set of items of the form AB, BC, CD, DE. That is, each item has at least one, and probably several,

elements in common with one or more other items, but no, or few, elements are common to all items" (p. 575).

Common indicators in speech behavior of a fuzzy set include the use of qualifying adjectives in labeling members, as when a color is called "off red" or "blue-green" (Kay and McDaniel 1978). Upon examination, such usages point the way to the identification of exemplars as "a prototype (clearest cases, best examples of the category) and nonprototype members, with nonprototype members tending toward an order from better to poorer examples" (Rosch and Mervis 1975:574). The "most prototypical members . . . are those which bear the greatest family resemblance to other members of their own category and have the least overlap with other categories" (pp. 598–99), "prototypicality" being "a function of the cue validity [or predictive utility] of the attributes of items" (p. 599).

With respect to "the New Guinea Highlands," it seems clear from the usages reviewed above that, despite attempts to specify attributes (elevation, population density, agricultural techniques, staple crops, settlement types, or social institutions such as ceremonial exchange), when anthropologists assign societies to "the Highlands" these attributes are less often truly diagnostic than loosely employed, with weightings on sliding scales. Thus, "the class of defining attributes that constitutes the intension of the term is not a class of attributes that are severally necessary and jointly sufficient, but a 'polythetic group' or 'imperfect community'" (Atran 1990:54). That this is evident to most is indicated by the grouping of societies into "core" (prototypic) and "fringe" and like extensions of category membership.

When anthropologists elaborate "the Highlands" category to accommodate prototypic ("core") cases and ("fringe") extensions through family resemblances, they do not thereby resolve all of the definitional complexities that this category entails. This is only partly because different writers still sort cases differently, depending on which attributes they highlight and how carefully they apply them. In fact, there is probably substantial agreement, at least regarding "the core." While groups that belong to the "core" have largely to be identified through a process of elimination (coming down to those that are not designated as "fringe") or by the frequency of their use as main exemplars, one could say that the language groups called Enga, Melpa, Kuma, Chimbu, and those clustered around Goroka and Kainantu are the ones most often regarded unambiguously as "Highlanders."<sup>5</sup> One important complication is suggested by the popularity of what might be called "the continuum game," recently played most comprehensively by Feil (1987) in his designation of several main "societal configurations" distinguished on the basis of their variable elaboration of ceremonial exchange or other "traits." When such continua are developed, even "core" groups are treated as constituting chains of societies linked through family resemblances with respect

5. Knauff (1990:275) is probably idiosyncratic in considering Siane "non-core" but apparently not "fringe."



to one attribute or another. A second complication is illustrated by the diversity to be found *within* language groups, especially in the north, such as "Enga" and "Melpa" (hence my use of quotation marks). While in most discussions "the Enga" *really* means Mae or Raiapu Enga, a cautionary reminder may be found in Dornstreich's (1974:475–87) careful charting of a "cultural typology of Enga-speaking peoples," in which he distinguishes "Central" from "Intermediate" and "Outer Enga," with the latter two kinds of "Enga" almost certain candidates for "fringe" status at best. The same could be said for northern "Melpa," whose ecological situation differs strikingly from that of better-known "Melpa" groups around Mount Hagen, leading Gorecki and Gillieson (1989) to include them in the northern "Highland fringe."

### The Utility of "the New Guinea Highlands"

Many anthropologists will perhaps be neither surprised nor troubled by the results of my review.<sup>6</sup> Feil (1987:6) seems content with "the rather vague concept of the highlands as a cultural-ecological unit," and Brown (1978:18) concedes that "the region which we consider here has no precise physical boundary, and any social boundary would be arbitrary, cutting social linkages and trade routes." For all its "fuzziness," it is virtually certain that "the New Guinea Highlands" will remain, in some sense, a "region" in which some people do their research and about which much will continue to be written—if for no other reason because of the social organization of our discipline or, as Fardon (1990:24) views it, because "regionalism is so pronounced a feature of our professional practice." As he elaborates:

Regional factors influence the entry (in the broadest sense) of the ethnographer to a field that is necessarily pre-imagined, the circumstances under which fieldwork will be carried out, the issues which have been preconceived as appropriate and pressing, and, in writing up, the canons of adequate reporting and the audience to whom, in part at least, the work will be addressed and whose opinions will be the most telling.

Unquestionably, in Melanesian studies, "the New Guinea Highlands" has come to have such influences, and those influences extend "outside the narrow circle of regional specialists. The most pervasive of these is the projection for non-specialists of regional representations (often via exemplary texts or, as commonly, secondary rescensions of them) which establish an image of place in terms of particular problematics which it typifies" (Fardon 1990:26). Thus, for many anthropologists the

"exemplary texts" regarding the "Enga," "Melpa," and "Chimbu," with their "big men," ceremonial exchange systems, and challenges to or refinements of "African models" of descent, have come to represent "the Highlands."

But clearly those who invest considerable energy in trying to establish contrasts between "the Highlands" and other "regions" and then theoretically to account for them believe that they are doing more than merely employing an arbitrary professional sorting device. In these "postmodern" times, it is perhaps not surprising that none of the writers discussed here uses the old-fashioned term "culture area," yet it would seem that when they write of "the Highlands" vs. "the Lowlands," etc., they are in fact invoking that concept: "Culture areas are geographical territories in which characteristic culture patterns are recognizable through repeated associations of specific traits and, usually, through one or more modes of subsistence that are related to the particular environment" (Ehrich and Henderson 1968:563). I will not rehearse here the problems whose cumulative weight played a large part in the near disappearance of the term "culture area" from contemporary anthropological discourse (at least outside of pedagogical contexts, where it still thrives), but it is worth noting briefly a few of the costs incurred by the attempts so far to employ "the New Guinea Highlands" as a theoretical or analytical construct.

First, such usages misleadingly imply greater environmental, social, and cultural homogeneity than can be demonstrated even *within* the prototypic main exemplars such as the "Enga," "Melpa," or "Chimbu."<sup>7</sup> As some (e.g., Strathern 1990) have pointed out, the "Highland" groups of Irian Jaya, where ceremonial exchange systems appear to be rare or absent, are routinely ignored, a failing that is doubtless attributable at least in part to the fact that much of the relevant literature is published only in Dutch or German. But even within Papua New Guinea apparent exceptions to depictions of the most celebrated cases are marginalized by creating "fringes" or "Mountain Papuans" and then treating them as if it were *they* that required some special explanation.

Second, "Highlands societies" are frequently portrayed, individually and collectively, with little if any consideration of the antiquity or historical stability of the "configurations" or institutions attributed to them (or to their foils in "the Lowlands" or wherever). While

6. An analysis such as this one could be carried out for other "culture areas" that are doubtless as "fuzzy" as "the New Guinea Highlands," such as "Amazonia," "the Northwest Coast," "Melanesia," and "Polynesia" (Thomas 1989); see also Knauff's (1993) superb dissection of "South New Guinea."

7. In urban centers of Papua New Guinea *hailan* (Tok Pisin for "highlands") is in frequent use as an ethnic marker. In my experience, this expression often connotes or is used interchangeably with "Hagen" or "Simbu" (Chimbu), just as Bougainvilleans in the 1960s came to form images of "Highlanders—of whom the Chimbu served as the prototype" (Nash and Ogan 1990:7). The growing literature on ethnicity in the Pacific (see, e.g., Linnekin and Poyer 1990) offers many fascinating examples of "regional" identities built in nonacademic discourse upon "fuzzy sets."

8. Such implications of homogeneity are often the basis of cultural stereotypes (e.g., "flamboyant" and "bellicose" yet "pragmatic" Highlanders), as Herzfeld (1984) has pointed out in relation to another questionable "culture area," "the Mediterranean."



it may be as difficult to conceive of "Highlands" peoples without sweet potatoes as of "Plains Indians" without horses, in both cases we are dealing with adoptions no more than a few centuries old. Feil (1987) has been attentive to pre-*Ipomoea* precursors so far as crops and agricultural techniques are concerned, but he bases his social reconstructions primarily on what we know from the "ethnographic present." With respect to the latter, few have considered with any seriousness the degree to which features often regarded as diagnostic of "Highlands societies," such as large-scale ceremonial exchange and "big man" leadership, were affected if not created by the colonial process (cf. Hughes 1978).<sup>9</sup>

Third, at the same time that writers are imprecise and inconsistent with each other in drawing the spatial boundaries of "the Highlands," their descriptions and analyses persist in treating the societies as if they or the "region" as a whole could be understood in isolation. This view can be traced to the first decade or so after the "opening up" of "the Highlands" to anthropologists. Thus, Read (1954:2) declared: "The Highlands of New Guinea form a region which is more or less isolated geographically from the surrounding country by high mountain ranges," and Watson (1964:2) extended the point beyond mere geography: "The pre-1930 isolation of the Highlands has proven to be more than a question of the lack of reports by literate explorers. . . . the area is anthropologically a good deal more than a region by default. The conditions that isolated the Highlands appear to have given it a distinctive character reaching well back into time." Perhaps such statements were intended only to stress the fact that fieldworkers were encountering, in the 1950s and early 1960s, societies that contrasted in many ways with those of the better-known "Lowlands" and islands. But, in any case, the image of "Highland peoples" as "isolated" has doubtless hindered our understanding of them.

Brown (1978:29) has acknowledged that "the small communities and fragmented groups of the fringe area . . . have always been intermediaries and traders, bringing new ideas and techniques into the highlands from the outside." But few attempts have been made so far to document systematically or include in explanatory efforts the "social linkages and trade routes" involved (Brown 1978:18). Yet it was precisely such linkages and routes that connected "the Highlands" with the north coast and even areas beyond New Guinea in trade in bird plumes (Healey 1980) and in shells and other marine products (Hughes 1977), the diffusion of tobacco (Hays 1990), and the movement (with localized transformations and permutations) of cults (Hays 1986). It may even be, *contra* Watson above, that such linkages have been instrumental in the development of what commonalities can be observed in many "Highlands" societies. As Brookfield (1990:69, emphasis added) notes

in criticism of Feil (1987) but with implications for all of us, much recent work

ignores the wider question of relations with the people of the lowlands and with other highlanders *through the lowlands* which are posed by the remarkable similarities between highland peoples several hundred kilometres apart and with no possible contact within the cordillera. It is simply not possible to write a credible account of the evolution of highland Papua New Guinea societies in isolation.

If "the Highlands," "fringes," "the Lowlands," and other commonplace abstractions are to signify something more than geographical regions, we need attention to all of these caveats, but at minimum we need explicit statements of *principled* criteria for bounding such categories, and we need to apply them in such a way that both inclusions and exclusions are clearly motivated and consistently executed. This does not mean that consensus or uniformity will necessarily result, for the criteria and resulting sortings will inevitably depend, at least in part, on factors extrinsic to "the region" itself. As "big men" vs. "great men," ritualized homosexuality, and sexual antagonism go in and out of vogue as theoretical foci, "core," "fringe," and "continua" will doubtless continue to be identified in terms of attributes or considerations that are particular to the agenda of the researcher. Anthropologists are human beings, and "fuzzy-set" and "prototype/extension" models, after all, flow from a general information-processing strategy (Atran 1990:55):

Prototypes facilitate the patterning of input for use in memory and for one's actual dealings with the day-to-day world by describing similarities among particularly useful, salient or familiar clusters of exemplars. Prototypical patterning is thus contingent on memory and use. Because memory and use are influenced by context, prototypical patterning transforms in accordance with changes in history and society, with the extent and nature of such transformations varying as much as individuals and cultures vary.

Definitional issues will remain, then, and boundary disputes will be a continuing feature of any "essentialist" approach to the peoples and societies of New Guinea and Melanesia. But quests for the "traits" that truly distinguish "Highlanders" from "Lowlanders" and for their correlates from which explanations for such differences can be developed are not the only kinds of explorations that can be conducted.

## From Morphology to Process

Essentialist approaches to "the New Guinea Highlands" as a "region" have relied upon the categorization of societies in terms of their "culture-bearing aspect." Thus, "core" and "fringe" groups, like "ethnic groups," have been distinguished "by the morphological characteris-

9. Even more recently, it was only in the 1970s that the Irakia Awa began to intensify pig production for purposes of ceremonial exchange (Boyd 1985); perhaps this will be sufficient for Whitehead (1986:87) to elevate them to "Highlander" status.

tics of the cultures of which they are the bearers" (Barth 1969:12). In this approach, "differences in groups become differences in trait inventories; the attention is drawn to the analysis of cultures, not of ethnic organization."

An alternative program would take as its primary object the social linkages that are in danger of being otherwise obscured. In Barth's terms (1969:15), "the critical focus of investigation from this point of view becomes the ethnic *boundary* that defines the group, not the cultural stuff that it encloses." With attention to boundaries and linkages, the "fringes" assume critical importance. One example of the kinds of linkages that have in fact long characterized the actual situation is to be found in the trade routes referred to above by Brown. As in Healey's study (1980) of the plume trade and Hughes's (1977) regarding salt, pigments, pottery, stone tools, and shells, in my own ongoing investigation of the spread of tobacco and smoking in New Guinea and the social dynamics by which it was effected (see, e.g., Hays 1990), categories such as "Highlands," "fringe," and "Lowlands" are irrelevant. Thus, speaking only of Papua New Guinea, I can now demonstrate the existence of trade networks involving tobacco which linked northern Chimbu with the Ramu Valley and Mae Enga with the Sepik foothills. In the south, I am able to document a vast system that joined the Huli with the peoples not only of the Bosavi area and the Strickland Plain but also of areas to the east as far as Lake Kutubu, the Kikori and Turama Rivers, and the south coast.

How can such linkages have escaped our attention for so long? To be sure, as Brown (1978:28–29) and others (e.g., Weiner 1988) have noted, much of our detailed knowledge of such peoples as those on the "southern fringe" has come only from recent fieldwork (though Williams long ago [1940–41] demonstrated the key role played by those in the Lake Kutubu area in linking the southern highlands with the south coast). But this recency of attention to the "fringe groups" may itself be due less to the fact that "they are sparsely distributed in relatively inaccessible areas" (Brown 1978:29) than to an essentialist view of such societies as "peripheral" or "marginal" in more than a geographical sense.<sup>10</sup>

These remarks are intended not as a plea for elevating

"the Highlands fringe" to academic equality with "the true Highlands" but as an indication of how little any such categories help us when we shift our attention from social *morphology* to social *process*—in other words, from nouns to verbs. I am proposing here a change of focus from what people *are* to what people *do* (e.g., trade, engage in ceremonial exchange, intermarry, fight) or, indeed, do not do. Such a shift can, in fact, help us arrive at better answers to *both* kinds of questions, for, as Barth (1969:10) has argued, "ethnic distinctions do not depend on an absence of social interaction and acceptance, but are quite to the contrary often the very foundations on which embracing social systems are built." These systems, such as those disclosed by studies of transformations in cults within particular geographical regions (e.g., Knauff 1985, Strathern 1991) or the "community of culture" suggested by linkages among diverse language-groups on the north coast of New Guinea (Welsch, Terrell, and Nadolski 1992), can incorporate deliberately maintained differences or boundaries as well as those that may be products of varying environments, resources, or local histories. It should be apparent that such systems can only be discovered and understood by tracing out the connections and boundaries of particular kinds of interaction—in what are sometimes called "village-outward" studies—rather than by imposing *a priori* a grid of traits to demarcate a "region."

Put simply, the "ethnic groups" of "the Highlands," "fringe," and "Lowlands" have long been engaged in numerous and wide-ranging networks of interaction, but the result has not been homogeneity or uniformity. Why not? Whichever "region" interests us most and however we choose to define it, we must wonder why it is not larger. Of course, constraints imposed by environment, climate, and disease may be a part of the answer so far as some traits (e.g., subsistence base and population density) are concerned (Brookfield 1964), but what about the rest? If it is people as much as "Nature" that create, maintain, or ignore boundaries, we need to know how and why, and for that we need new ways of framing our questions.

## Comments

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I must respond, for my name has never before been cited so many times in such a short space. Hays can fairly say that the central highlands of Papua New Guinea, as a geographical-cultural region, was invented in the 1950s and 1960s. Read (1954) was certainly influential. Historically, it might also be said that it is a region by virtue of discovery and settlement by Europeans after 1930. There was a period, about 1946–50 I think, when "Central Highlands" was a district and the name used to iden-

10. Richard Scaglione (personal communication) has reminded me of a parallel in the history of anthropology to the east of New Guinea, where some parts of Micronesia and the Solomon Islands have long been categorized as "Polynesian Outliers." Such marginalization may be understandable from the viewpoint of Central Polynesia, with its large chiefdoms and even kingdoms, but from a prehistorian's perspective the situation can look quite different (Terrell 1986:120–21): "Ironically enough, we are now beginning to see that the true outliers of the Polynesian realm may not be the Polynesian-speaking communities found on the fringes of Melanesia and Micronesia after all. If there are outliers in the Pacific—places off the beaten path and away from the main arena of Pacific prehistory—the real Polynesian outliers are more likely to have been those islands, large and small, geographically situated at the distant corners of the great Polynesian triangle: Hawaii, New Zealand, and Easter Island."

tify the area. I can remember the first time I heard about the highlands: In 1956 I had just arrived in Canberra. The newly formed Association of Social Anthropologists (Australian branch) brought together Australians who had recently completed fieldwork in the highlands; the discussion, typical for social anthropology of the time, centered on terminology for descent, groups, and communities.

At the Australian National University the Nadel program, to be honored posthumously, was to focus on the New Guinea highlands because of the concentration of population and the excitement of studying people whose areas, social systems, and response to contact could be traced while Australia brought them into the modern world. The ANU was then competing (if I may let it out) with anthropological research programs at Sydney and Washington (and Mick Read soon went there); research and writing grew quickly. There were also some geologists, geographers, mission anthropologists, and others studying the region. I got my chance for Chimbu fieldwork on social organization and social and political change a year or two later; at that time Harold Brookfield and I joined to study agriculture, land use, and economy.

The 1964 papers edited by Jim Watson were a major step in establishing the region, and then defining papers by Meggitt, Brookfield, and all the rest drove the regionalism ahead. The "fringe" (Mountain Papuans, Ok, and Anga) was hardly known. Irian Jaya was a key area of study in the 1950s and 1960s under the Dutch; after it became part of Indonesia, research permits were difficult or impossible to obtain, and Anglophone anthropological research persevered in Papua New Guinea. The linguistic studies of Wurm began at the same time as Brookfield's and mine (we shared a Jeep purchased by the ANU in 1958).<sup>1</sup>

"Core" or "center" was variously defined and mostly confined to Australian New Guinea, which differentiated highlands from "fringe." As late as the mid-1970s I could find little ethnographic information<sup>2</sup> about them, for these areas were accessible to researchers only after the Australian administration had established a patrol post. There are still few road connections to much of the area.

If, then, we had invented a category or cultural region, what could have been the rules for inclusion and exclusion? None of the volumes of collected regional essays, even those with "New Guinea Highlands" in the title, were restricted to what is now recognized as the central Highlands. I can think of over a dozen such collections, on topics as wide-ranging as politics, religion, kinship,

sexual antagonism, gender, inequality, leadership, law, marriage, initiation, sorcery, and history, in addition to general essays and books which cover all or part of Melanesia.

It may be no more than a convenience in defining a region for teaching purposes and research area identification—textbooks, the organization of lectures and assignments, reviewers of research proposals and manuscripts—but I think that the highlands may have more in common than some other purported regions in Melanesia.<sup>3</sup> Highlands intensification of agriculture has combined sweet potato subsistence with pig raising to make massive feasts possible in the area from Chimbu to Enga and again in some sections of West Irian. This pig-feast region<sup>4</sup> is surely not a continuous area and not commensurate with the highlands, as the Eastern highlands are mostly left out.<sup>5</sup> Anthropologists have made some progress in defining Sepik and Massim cultural regions, which, like the highlands, seem to be convenient categories for teaching units, symposia, and essay collections. Is that a good enough reason?

Hays says, "Many anthropologists will perhaps be neither surprised nor troubled by the results of my review." He is right. As research in these interstitial areas (that is, between the "seaboard" and the highlands) has been published, we have learned two important things about relations between the highlands peoples and their neighbors. The first is the importance of trade and the movement of materials and goods (Hughes 1977); agriculture, pigs, and sweet potatoes were surely crucial in making highlands culture. The second is the extent and importance of intergroup relationships—exchange relations which cross regional and linguistic boundaries, the desire for and acceptance of new practices, including cults, and payment to outsiders for the privilege of holding certain ceremonies or rituals. Through these studies we may downplay the artificial categories erected in the past generation and gain an understanding of intergroup connections.

I think, however, that Hays and I may want to draw different conclusions. He is dissatisfied with fuzzy sets, while I would now ask: what purpose would be served by clear ones? If we attempt to create exclusive regional categories and culture trait lists, we falsify all we know of cultural influences and change, relations with neighbors, and intercultural interactions.

1. This work established a language phylum which included some peoples in Irian Jaya, and my comparative discussions often included these.

2. A manuscript editor hired by Cambridge University Press at first attempted to integrate my discussion of "fringe" (1978:28–39) with the general text. I wonder what would have been the meaning of "Highlands" if I had not objected to this reorganization and insisted that my chapter be reinstated.

3. When we held the symposium "Man and Woman in the New Guinea Highlands" at the American Anthropological Association meeting in 1974, there were several papers which were not included in the later publication, and Kelly's paper was added to it (Brown and Buchbinder 1976). Perhaps we should have changed the title. At the time of the symposium, Rhoda Métraux protested that the highlands seemed overrepresented in symposia, with the Sepik left out; I suggested, and she organized, a Sepik symposium (Métraux 1978).

4. Another way to look at it is Feil's (1987), whose continuum peaks in the Enga Te pig exchange.

5. If large feasts were held in the eastern area before intensive contact, they have not persisted. I wonder if the influence of the Seventh-Day Adventist mission can have been decisive.

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Hays does Melanesian studies a service by examining the inconsistent and contradictory ways in which Melanesianists have used the term "New Guinea Highlands." His paper should prompt them to think more carefully about the terms they use. He argues persuasively that "the Highlands" is a fuzzy or polythetic category, but I am a little bothered by his apparently assuming that such categories are Bad Things and need to be expunged from anthropological discourse. Surely, the point that cognitive psychologists such as Rosch are making is that virtually all human thinking is done (and done very successfully) in terms of prototypes and fuzzy sets. All concepts in anthropology would probably turn out upon analysis to be just as fuzzy. In short, while I agree that we need to think more carefully about the categories we use, I do not accept that we should try to stop thinking in categories altogether as Hays seems to suggest. But of course we must suspend judgement on his approach until he demonstrates its superiority.

Hays's objection to the use of "Highlands" as a construct is that it has led ethnographers to misrepresent these societies as homogeneous, ahistorical, and isolated from the outside world. Although not a Highland specialist myself, this seems to me unfair. I doubt that many Highland ethnographers will agree that trade networks have up till now "escaped our attention" or that few have considered whether big men and large-scale ceremonial exchange "were affected if not created by the colonial process." A non-Melanesianist reading this article could gain the impression that New Guinea Highland ethnography had been stuck theoretically in the 1940s and 1950s until the publication of this paper. Hays seems to some extent to have "essentialised" the Highland ethnographers, imputing to them that very homogeneity, changelessness, and isolation which he accuses them of attributing to the Highland societies themselves.

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The more I read Hays's paper the less I could refrain from smiling. His brilliant analysis of how anthropologists have dealt with "the New Guinea Highlands" amuses me, but the questions he raises are sobering.

The expression "culture area" is simply out of fashion for both modernist and postmodernist anthropologists, even though most of them obviously have a similar notion in mind in speaking of a "region." At least, it seems that none of them has a better concept to offer.

Detailed and systematic comparative studies on New Guinea have become fewer and fewer in the past 20 years. Thus, methodological and theoretical questions

about the framework for comparison, mostly touched upon only in passing, since the focus of most publications is on something else, have been widely neglected—except that those who study material expressions of culture comparatively have never been able to avoid them.

Tiesler (1990) offers an admirable attempt at systematic classification of New Guinea art and an excellent summary of how different scholars (since Haddon 1984) have approached this problem. Some of these anthropologists have taken "style" as a starting point (e.g., Speiser 1937, Gerbrands 1951), and Speiser has related styles to historical classifications. Another approach links considerations of style primarily to geography (Bühler 1961). Tiesler points out that since Bühler there has been no attempt to develop theoretical and methodological concepts for classifying art in New Guinea. In his introduction he formulates the problem on a general level: "Die Erarbeitung von Gliederungsprinzipien für die Vielfalt der Erscheinungen" (1990:235). I think this is the crucial question for those who do not want to limit their efforts to one specific culture. How can we work out principles of classification for the diversity of cultural phenomena?

If we acknowledge that this question is legitimate, a large range of possibilities arises. Hays's suggestion that anthropologists shift the focus "from what people *are* to what people *do*" is simply one possibility of many for structuring the continuum of cultural phenomena the better to understand certain aspects of it. It is obvious that any classification involves the construction of a grid to distinguish between aspects to be compared and others. Therefore, our attention should shift first and foremost to methodological questions: why we classify, what criteria and methods we use to attain this goal, and, finally, whether the system of classification chosen is consistently applied. "Commonsense abstractions" such as "the Highlands," their "core," and their "fringes" are classifications. If more attention had been paid to methodology, the comparison of cultures would perhaps have produced classifications in "the Highlands" that made more sense.

In shifting to "what people *do*" Hays identifies his perspective for structuring complex reality, but this approach is not new for New Guinea. Again, Tiesler published (in German and therefore probably unintelligible to most anthropologists working on New Guinea) a large-scale regional analysis of trading and exchange networks along the north coast of New Guinea in 1969–70. It answers the question what social processes lead to the development of a culture area and how one can be geographically defined on the basis of complex intertribal relationships depending on the goods traded and exchanged. Apart from this kind of analysis there are many other possible ways of identifying similarities and differences between cultures. One of the most commonly used is "languages" and their interrelations; these classifications, often taken as mirroring reality, are constructed, too, mainly on principles of lexicostatistics. A further way would be to ask people how they

classify themselves and others. And even if we go back to the notion of culture areas and traits, it is no longer in order to make cumulative lists of identical material objects throughout an area. Asking questions about similarities, differences, transformations, and "breaks" (*Brüche*), as I have tried to do with ceremonial houses in northern New Guinea (1989), reveals new insights that cannot be achieved otherwise.

Therefore, I wonder who might be able to decide what kind of classification is the right one without taking into consideration the context and the purpose for which such studies are made.

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My understanding is that "the New Guinea Highlands" (and, before that, "the Central Highlands" or "Central North New Guinea" [Nelson 1982:121]) was originally a set of colonial administrative and political boundaries and eventually came to serve the needs of government officials, gold miners, census takers, various exploratory patrols, and anthropologists. Hays argues that since those days it has changed from words on an administrative map to a murky anthropological concept. He ably demonstrates how confusing that concept is, and I am left wondering whether the Awa in the Eastern Highlands Province, whom I studied, are Highlands, fringe, or Lowlands people. Their gardens and some houses are scattered over 1,000 m in altitude; some of their behaviors resemble those of other Highlanders and some do not. Perhaps they are all three—or none at all.

Hays raises but does not address the additional epistemological problem of how concepts in human language can ever adequately represent "reality." He suggests that "the New Guinea Highlands" should more accurately be considered, because of its physical and cultural heterogeneity, as a fuzzy set. I am not thoroughly convinced that this is not some kind of semantic sleight-of-hand and that the older concepts of "region" or "culture area" did not allow for intra-areal heterogeneity and flexible borders. But a further problem arises when one tries to elucidate what specific traits, characteristics, or behaviors are associated with one fuzzy set and not another. Conceptually, "the New Guinea Highlands" and "the fringe," for example, are not equivalent to "red" and "off-red." Cultural characteristics may change, interact with one another, appear and disappear in ways that color categories do not. I do not find the idea of a fuzzy set more explanatory than the older terms.

Hays emphasizes that anthropologists should concentrate more on process than on morphology, on what people do rather than on what they are. Assuming, then, that we proceed with the notion of a fuzzy set, what specific traits should be found in the list? Are we to omit factors such as altitude, population size and density, and abstractions such as patrilineality because people don't do these things? (Or do they?) Should we look instead at

warfare, ceremonial exchange, and political leadership, which are more obviously behaviors? The Awa practiced warfare (Hayano 1974), as did the Hageners, Simbu, and others, but there are vast qualitative and quantitative differences amongst them. A simple "absence of/presence of" coding of behaviors to discern which activities might be characteristic of Highlands, fringe areas, and Lowlands as fuzzy sets will produce similarities that are superficial and mask differences that are critical.

In a sense Hays's paper is an anachronism. His issues should have been raised 30 years ago, or earlier, soon after the Highlands first opened to anthropologists. Granted, the data were not complete then, and looking at these newly discovered peoples as a continuum from the coast was apparently not as important as portraying them as remote and untouched. That was the bias of anthropology. Part of this bias also shows in Hays's paper in the discussion of social processes such as trade. Trying to recapture an idealized model of Highlands societies at some generalized, undefined (but apparently precolonial) point in time would seem to overlook more than a half-century of history and change.

I would include on a more contemporary list of social linkages the irreversible changes wrought by the colonial and postindependence governments. How about groups that grow coffee, drink alcohol, gamble with cards, work on plantations, join the army, go to the university, travel as tourists, drive Toyota pickups, watch videos? Here the social linkages (modern "trade routes"?) between village, township, and city can be seen as a complex pattern of behaviors dynamically created in a much larger national or world system. If we focus solely on tradition and timelessness and ignore culture history and the contemporary, our academic view of Papua New Guinea will undoubtedly remain fuzzy.

We seem to have come full circle: the New Guinea Highlands is a convenient locational term for designating known political/administrative areas but severely flawed as a useful analytical concept. I applaud Hays for raising this issue, but he has not resolved it.

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Hays suggests that the comparative ethnography of the New Guinea Highlands has failed to address the issue of how the local inhabitants create, maintain, and ignore boundaries and that to do so we need "new ways of framing our questions." Given this conclusion, it is surprising that he does not draw on Marilyn Strathern's *The Gender of the Gift* (1988). Not only is it a comparative and synthetic account of Melanesian ethnography (with its specific emphasis derived from a "highlands"/Mt. Hagen point of view) but it is directly relevant to the main theme of Hays's paper. In fact, his closing words could in many respects be read as the starting point of Strathern's book.

Most of Hays's conclusions with regard to the comparative accounts of the "Highlands" have been rehearsed in the literature he cites, but he has done us a service in bringing them together systematically. To summarise: (1) It is difficult to reach agreement as to what constitutes "the Highlands" when analysis is based on single and/or multiple factors (e.g., environment, demography, economics, etc.). (2) From one vantage point societies may appear similar, but on closer inspection this seeming homogeneity dissolves, leading to the identification of "core" and "marginal" areas. (3) Most anthropologists nevertheless acknowledge the continuing usefulness of the concept. (4) Its vagueness or fuzziness is part of a more general cognitive condition shared by Highlander and anthropologist alike—in other words, unavoidable. What can be avoided, however, is definition of "the Highlands" in essentialist terms, which has focused attention on issues of morphology to the relative neglect of inter- and intraregional process.

An alternative strategy proposed by Hays and recently acknowledged by others (cf. Gell 1992) is to give greater attention to linkages between areas formed through trade. One reason for the relative lack of scholarly progress in this area is the conditions generated by the advent of a colonial and mission presence throughout Melanesia: trade became less apparent because of the influx of both European manufactured goods and traditional goods imported by Europeans. The energies and interests of local inhabitants were focused elsewhere. As urban centres, cash-cropping, and the circulation of money became established, for example, a transformation occurred in the objects deployed in ritual and ceremonial exchange (cf. Hirsch 1990). Nevertheless, the existence of wide-ranging trade networks has been established for particular valuables by Healey (1980) and Hughes (1977), and Hays (1990) has traced a link between the Huli of the Southern Highlands and the populations of the south coast. This is evidence for long-standing linkages between areas often thought of as separate and relatively autonomous.

Hays asks rhetorically why the result has not been homogeneity or uniformity, but I contend that these wide-ranging linkages have in fact produced a kind of homogeneity and uniformity. This is where I see Marilyn Strathern's account as being of central importance. On the basis of a comparative analysis of ethnographic material from diverse "regions" of Melanesia, she has suggested that Melanesian societies share an aesthetic (1988:341). She warns against assuming that this is the residue of a common past (p. 342). Rather, it reflects these societies' being outgrowths and developments of one another—implicated in one another's history. "Specific forms come not from generalized ones but from other specific forms" (cf. Kulick 1992 for linguistic parallels). Thus the wide-ranging trade networks described by Hays at once affirm these shared conventions and sustain them from diverse perspectives. To phrase this in terms closer to Strathern's, they highlight distinctions and obscure the common set of conventions that underlies them (cf. O'Hanlon 1992).

We cannot account for the emergence and operation of the boundaries/differences adduced both by anthropologists and by the inhabitants of Melanesia until we have grappled with the nature of these shared conventions. We must also be aware of how our Western conventions impinge on what we come to see as a "problem" in the first place (Strathern 1988:318–23). Harris (1986) has argued that repeated attempts to account for the origin of writing over the centuries have failed for the simple reason that no one has provided an adequate answer to a preliminary (but largely taken-for-granted) question: what is writing? Similarly, we cannot trace the historical consequences of the wide-ranging "interactions" highlighted by Hays by assuming that Melanesian sociality is a version of Western sociality; we must first establish the conventions implicit in these interactions. Hays is arguing that instead of looking at Melanesian societies or regions we should examine the links between them; Strathern is suggesting that we call "societies" and "regions" in this cultural context have a shared feature: the linkages at once produce and are a product of this commonality.

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Hays tackles the imprecision of the "Highlands" category in Papua New Guinea ethnography and criticizes recent comparative work centred on this. Exposing weaknesses in our customary ways of lumping cultures together, he shows that anthropologists sometimes disagree about which cultures count as Highlands cultures and that we are often unclear about the contents of the package with the Highlands label. Worse, the Highlands designation may blind us to linkages across zones, obscuring wider processes—trade, for example. Reifying the Highlands category may also produce a sort of "secondary Orientalism" by a reflex lumping of remaining Papua New Guinea cultures as "the Lowlands." For these reasons I think Hays has done us a service, and much of what he says fits with contemporary efforts to dissolve old anthropological categories. But despite fears of making essentialist mistakes I think we can continue to find uses for the Highlands category and others of its kind.

Hays talks about the trouble we have deciding where the Highlands begin and end, but maybe this isn't as bad as he thinks. For one thing, it's not clear that we need to think of the Highlands as being sharply circumscribed after the fashion of provinces or states—we've known about dialect chains, and so on, for a long time, and the fuzziness of the Highlands set should not in itself throw us off balance. More interesting, however, the fuzziness Hays discusses is not uniform, and I suspect that most of us would perceive some of the edges to be relatively sharp (e.g., the boundary between eastern Highlands and the Anga peoples). I think we should be curious about such things, and differentiating between sharp edges and



hazy zones is encouraged by attempts to describe cultural areas.

I also think we can learn a lot by asking more questions about where the notion of the Highlands comes from and what kind of life it leads today. For example, a look at Papua New Guinea's history shows that the extension of colonial control into the Highlands forced a massive reorientation of colonial policy (see Downs 1980 for details). This was not merely a matter of geography: the density and scale of the Highlands populations quickly led to administrative arrangements marking the Highlands off as a region distinct from the rest of Papua and New Guinea, a history mirrored in today's provincial boundaries. Not all mountain populations—for example, the peoples west of the Strickland or in the Owen Stanleys to the east—elicited this kind of treatment. This suggests to me that historical contingencies played a role in shaping our notions of the Highlands but also that the existence of the Highlands as a region is not simply the product of anthropological pigeonholing.

I can think of at least two other uses for the Highlands label, and following these lines up might be more difficult if we decided that there was no wheat lurking among the chaff. The first use is historiographic: any account of anthropological work in Papua New Guinea that failed to recognize a "Highlands period" would surely be missing something big. Postwar Melanesian ethnography moved to centre stage when the New Guinea Highlands were found to be located outside of Africa (Barnes 1962). The second use turns on the suggestion that we make a suitably indigenous form our focus by turning our attention to the Pidgin term *hailans*. *Hailans* is a Papua New Guinea folk category that has acquired a life of its own quite independent of what anthropologists have to say; looking to its regional and (novel, fuzzy) ethnic import may serve as a useful diversion from the ethnographically parochial preoccupation with My Village (vs. Yours).

In the end, I would be reluctant to do without the Highlands label because I think it can tell us something real about the cultures we are looking at—cultures which, after all, are not all entirely unique or equally different from one another.

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Hays's premise that the New Guinea highlands is a fuzzy set is compellingly documented and beyond dispute. Below I push farther the implications of his pregnant analysis and explore its inherent tensions.

"The New Guinea Highlands" is obviously a prototype that has been configured differently to fit various researchers' analytic agendas. The question Hays raises by implication is important: what larger historical and theoretical biases have informed it? As Appadurai (1986, 1988) has suggested, the pairing of name and place labels in anthropology itself has a history. At particular pe-

riods, certain areas become "hot" for studying current topical and theoretical issues. A given topical emphasis can easily be reified and magnified by ethnographic history, itself coming to define the region. The first major monographs become "classic" and imprint the perception of later students and comparative researchers. Thus, in the 1950s to 1970s, "the New Guinea Highlands" became a hothouse for studies of big-man politics, competitive gift exchange, pre-state warfare, loosely structured clanship, pre-state agricultural intensification, and sexual antagonism. These features *were* in various guises present there, but their reification as anthropological categories tended to configure "the Highlands" as a static precolonial ethnographic region defined in significant part by opposition to other areas of Melanesia in which its "typical" traits were assumed to be attenuated or absent. Thus "the Highlands" is configured differently to contrast particular patterns of leadership, gender, exchange, or subsistence with those in the Sepik, lowland south New Guinea, the Massim, and so on (e.g., Lawrence and Meggitt 1965; Herdt and Poole 1982; Lindenbaum 1984; Whitehead 1986; Feil 1987: chap. 7). As the *Annales* historian Marc Bloch recognized (1971[1913]:122), the very notion of a region depends on the theoretical problems one is concerned with. Without denying the importance of comparative studies, these interregional contrasts are increasingly being questioned and put in historical perspective (e.g., Godelier and Strathern 1990; M. Strathern 1990; A. Strathern 1990; Knauff 1993).

Hays's point that fuzzy-set regions call for *more* rather than less specification of concrete ethnographic contours deserves emphasis. It points to a creative tension in his paper between the use of principled criteria to identify ethnographic regions and the tracing of networks and processes that crosscut them. Exactly how these competing views should be balanced is a key and unanswered question. Hays appears to advocate the latter, but too much emphasis on these networks carries the danger of obliterating what *is* distinctive to particular ethnographic regions.

One approach might be neither to assert dogmatic regional boundaries nor to ignore them but rather to be clear why a given geographic region is appropriate as a unit of analysis. Not all the interesting ranges of variation can be supposed to line up within a certain region. This reflexive move can at the same time allow us to be more rather than less empirically specific in our comparisons and contrasts (Knauff 1993).

Much current disagreement in the assertion of regional characteristics and definitions comes from conflict over scales and purposes of analysis: large-scale generalities and characterizations seem inadequate to characterize a region when its range of internal variation is more closely considered and the scale of analysis is reduced. Further, it would be foolish to be constrained by regional contours appropriate to previous ethnographic interests or time periods, because indigenous networks and regions themselves proliferate and change. That regions like the Highlands are fuzzy means neither



that all ethnography is relative nor that inalterable boundaries must be imposed. Arguably, it is important to maintain a creative tension between the heuristic delineation of regions and the analysis of networks and processes that crosscut them.

Hays's paper is effective in raising issues and possibilities; it is now up to us to develop and concretely use them.

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As Hays argues, the use of regional categories such as "the New Guinea Highlands" in anthropological works implies (and too often asserts) that the peoples referred to (1) are culturally homogeneous (that is, their communities are "similar"), (2) share (in some sense) historically stable, autochthonous traits, and (3) can be understood as isolates (that is, studied as if their "linkages" with outsiders were irrelevant). I applaud Hays for his persuasive demonstration that we have no basis for considering "the New Guinea Highlands" an ethnographic "region" in these senses, but I urge him to take his critique farther. Indeed, heterogeneity and shifting, boundary-engendering "linkages" are to be *expected* given what we know about Melanesian symbolic and social inventiveness (e.g., Wagner 1972, 1981, 1991; Strathern 1988). What is more, we should expect cultural transformations at *all* orders of regional magnitude (as Hays hints in his remarks about the difficulty of identifying coherent cultural "cores" such as "Enga"). Melanesia may be even more, and more interestingly, heterogeneous than he thinks.

Hays is right to reinforce the point (also made by Appadurai 1986, Fardon 1990, and others) that our discipline encourages us to reproduce analytically loaded regional categories. He believes that we would be better served by a shift from "morphology" to "process." This is an attractive suggestion on its face, but we need to recognize that *the same problems* Hays has identified in our treatment of "morphology" also exist with respect to "process" (and, conversely, that there are better approaches to "morphology" than those Hays criticizes). Our sharpest (but dangerously recursive) challenge is to expose and then rework the relationship between our own comparative discourses (whether about "morphology" or "process") and indigenous social practice.

Hays has high hopes for "process." In studies of social "linkages" regional distinctions like Highlands/Lowlands become irrelevant, and our attention shifts from family resemblances and the like and to "difference" and "boundaries." Hays reaffirms Welsch, Terrell, and Nadolski's (1992) important point that social interaction may deliberately reproduce difference in New Guinea, and he asserts that in order to understand why regions (zones of similarity) are so small we need to understand why and how people create and maintain boundaries.

I agree thoroughly with the importance in Melanesia

of, shall we say, making a difference. Narrowing Hays's prescription, I would suggest that we center our attention on *indigenous comparative discourses*, a topic already elaborated in studies, for example, of "gender," "exchange," and "myth" (Lederman 1990a, b, 1991, 1992). When we do so, we learn that the "same"/"different" relation is organized and deployed "differently" in local practice and in anthropological analysis. Understanding this means reworking our whole comparativist game: if our "medium" ends up obviating our (their?) "message" ("medium"?), at the least we ought to have intended that effect! Failing to understand it, we run the risk of "essentializing" social processes just as Hays suggests we have "essentialized" regions. Social processes will also be misrepresented as homogeneous, ahistorical, and systemic so long as we insist on presenting their meanings as unitary, adjudicated, and authoritative. Having downed the disciplinary dragon of Regionalism, we may find ourselves set upon by the social-theoretic snake of a certain kind of Objectivism (Bourdieu's "practice" being of no great help here).

"Trade" is a good case in point (Lederman n.d.). As long as we take it upon ourselves to determine that an interaction *is* "trade" and then go on to compare it with "similar" acts elsewhere, it will end up being as incoherent an analytical category as "the New Guinea Highlands." But we already "know" better (even if we don't always recognize the knowledge as such). A reading of any number of monographs (e.g., Malinowski 1922, Gewertz 1983, Godelier 1986, Healey 1990) reveals that one person's "trade" may be his partner's "gift exchange"; that one person's asymmetrical exchange may be the other's reciprocity; that one's party's external boundary may be the other's internal relation. Mutual mistranslations and reinterpretations of one another's social forms is mundane. As a step towards understanding how Melanesian social process maintains "difference" (rather than producing hegemonic or encompassing structures of relationship or larger "regions"), we need to acknowledge the *typicality* of decentered, asystemic transformations in our accounts in the very ways we juxtapose local constructions of events and interactions.

The question, then, is not why this or that region "is not larger" but how any "region" is made to appear in the first place. The same goes for "processes." In answering these questions, we must take care to distinguish our own discursive interests and those of Melanesians. Then we might advance Hays's critical contribution by exploring the potential of different disciplinary writing styles to "translate" these interests with the subtlety they demand.

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Hays makes the important and persuasive argument that no criterial feature or set of features will allow us

to delineate unambiguously the internal characteristics or limits of "the New Guinea Highlands." Though analysts often settle on a variety of features ranging from elevation to ceremonial exchange and pig husbandry, these features are neither diagnostic nor predictive. The crucial implication is that those who believe that "the New Guinea Highlands" is anything more than an analytical construct are confusing the logic of science with the logic of social formations.

Hays's paper calls attention to a submerged but long-standing problem in the anthropology of New Guinea societies: though the character of its analytical object often deprives it of a sense of limits, theory and method presume that real limits exist and that anthropologists can craft notions of regional and cultural closure. Oppositions such as Highlands versus Lowlands, fringe versus core, and terms such as "external and intercultural" are the epistemological instrumentation for attaining such closure. However, this viewpoint not only takes too much for granted but masks the process of construction of cultural/ethnic identity. The Maring (usually cited as a fringe group) offer a critical example. Collective identities such as Maring (an imposed rather than indigenous name) as opposed to specific clan and clan-cluster identities did not exist prior to the incorporation of Papua New Guinea "within" a nation-state (such as Australia). Further, the limits or boundaries for a given culture and language are characteristically graded. The clan cluster at Kandambamp is comprised of both Maring and Kalam (culturally similar, linguistically distant) peoples. In this regard, Maring culture flows into and overlaps with Kalam culture (LiPuma 1988), and the same may be said for Maring and Manga culture at the other end of the valley.

There are several ways for anthropologists to deal with the issue of closure. If closure is the objective, Hays maintains, we need explicit principled criteria for bounding these categories, and we need to apply them in a systematic way so that both inclusions and exclusions are adequately motivated and consistently executed. Such a search for limits stems from the a priori assumption that structural and functional linkages between "highland" and "lowland" societies, between one culture and another, and between the central and peripheral groups that comprise a given culture are external rather than constitutive relations. This is problematic precisely because what we need to determine is how the relations between groups, cultures, and regions generate structures that permit socially objective categories to emerge.

Hays concludes that we need new ways of framing the issues. He asks what would happen if we rejected essentialist approaches to "the New Guinea Highlands" and stopped searching for diagnostic characteristics. He suggests that we shift from social morphology to process, changing our analytical focus from "what people are to what people *do* (e.g., trade, engage in ceremonial exchange . . .)." But this reformulation (derived from Barth) does not, I would submit, go far enough or answer the critical questions that surround the issue of limits.

First, focusing on what people do, while certainly central, can never explain the organization of trade, the form of ceremonial exchange, or other structural relations. When agents make an exchange or negotiate their identity they do so from a determinate position within an objective social field (e.g., the definition and history of a clan and the dispositions inculcated in clan members). Second, focusing on action cannot explain the construction and politics of identity, how peoples come to identify and represent themselves as Simbu, Kuma, or Enga, and how terms like "Highlander" have entered into the political discourse and the making of the Papua New Guinea nation-state. It also cannot explain why a certain politics of identity (e.g., a *vitae* which specifies "the New Guinea Highlands" as my area of expertise) is vital to anthropological discourse, recognition, and position taking within the field.

Analysts who focus on what people are and those who focus on what people do have this in common: they both grasp social practice in terms of what is directly given to ordinary experience. But what if "the New Guinea Highlands" were more akin to a solar system in which the orbit of any one culture is defined by the gravitational pull and push of all the others? What if the reality of the Highlands were nothing less than a set of cultural spaces defined by their interaction? On this view, "the New Guinea Highlands" would not be reducible either to objective criteria or to what people say about themselves (e.g., defining themselves as Highlanders), and the aim of analysis would be to construct the ethnographic space that would allow us to grasp the wide range of variation observed throughout Papua New Guinea. In a relational analysis such as this, there would be no such thing as a peripheral, marginal, or fringe society; there would only be societies that more or less share degrees of sameness and difference. Though the notion of fuzzy sets is an improvement on conventional views, I would argue that the trope of "sets" itself should be abandoned. Pushed to its limits, an anthropology of Papua New Guinea does not need to focus on what people do as opposed to what they are; it needs to dissolve that very distinction. The "people" in "what people are and do" includes anthropologists.

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Hays's thoughtful, scholarly critique of some of the anthropological writing about Papua New Guinea could not come at a more appropriate time. Pacific anthropology is hardly immune to current attacks on essentialist portrayals of other societies or large sections of the world. Indeed, concepts once considered as basic as that of "Melanesia" seem now to obscure rather than illuminate our studies (e.g., Green 1991). What makes Hays's contribution valuable is that he does not simply criticize but also points to ways in which our research might be better focussed.

I am not normally sympathetic to importing terms from other disciplines when anthropology is already bursting with jargon, but "fuzzy set" seems benign enough, especially since Hays makes clear that this is a general human "information-processing strategy." Rather than attempt to answer the question in the article's subtitle, however, I would prefer to underscore and amplify slightly in a comparative context the points made about history and process in Pacific ethnography.

A failure to take adequate account of historical processes in ethnography is hardly confined to New Guinea, although studies of the area provide more examples than Hays has time to recount. (A particularly telling one appears in Godelier's film about the Baruya; my undergraduate students are always amused by the anthropologist's embarrassed admission that only belatedly did he learn that the large pig herds he counted were a recent phenomenon, occurring only *after* the introduction of steel tools.) The argument of a recent volume edited by James Carrier (1992; see also Keesing 1990:158) is precisely to highlight the inadequacy of descriptions of life in New Guinea and the Southwest Pacific generally that do not perceive and analyze those cultural features which certainly reflect the incursions of a larger world system.

However, as Hays makes clear (e.g., in his citation of Hughes 1978), innovation and change did not wait for the arrival of Westerners to shape the cultures of Melanesia (if one may still use that shorthand term). The isolated Stone Age tribe ignorant of any other humans may still have a place in supermarket tabloids, but it has no place in ethnography. Inasmuch as modern ethnography begins with an account of interisland exchange in New Guinea waters, it is embarrassing that today's practitioners still need to be reminded "to view the tribal world as comprising regional systems" (Keesing 1990:153). Components and links within such systems are an obvious place to start investigation.

Perhaps more pressing than any problem of drawing boundaries believed to reflect "the real world" in New Guinea or elsewhere is that of drawing the most useful boundaries around our descriptions. Even if we follow Hays's sound advice to approach certain issues from a "village-outward" perspective, it is not clear, at least to me, what shape(s) our ethnographies might then take. One can easily see that international markets link Bougainville cocoa growers to the Chicago Board of Trade, but it is hard to visualize the appropriate way to write about that kind of system without losing sight of those islanders whose stories we most want to tell. Nor do I find much help in the pronouncements of those who most loudly claim to know the answer (e.g., Clifford and Marcus 1986).

Nevertheless, the challenge to move from the ethnographic present to historical understanding—from morphology to process in Hays's terms—cannot be ignored. For those who would meet this challenge in writing about the Pacific islands and their inhabitants, salutary ground-clearing exercises like the present article are most welcome.

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Hays's insertion of detailed definitional checking and cognitive set theory into the discussion of the "Highlands societies" is welcome and timely, functioning as a device for clearing the ground for another approach, the study of process rather than morphology. I add here three comments.

The creation and acceptance of "the Highlands" as a region may owe much more to administrative (and subsequently interethnic) usages than we have tended to notice. "The Highlands" was originally associated with the patrols of J. L. Taylor and the Leahy brothers and its extension marked in a sense by the limits of those patrols; tacked on were the patrols of Jack Hides into what later became known as the Southern Highlands. Later, in postwar years, the area was defined by the creation of a Highlands District, which was then progressively subdivided. The ethnographers who were first permitted to work in this area themselves by accident provided the "cultural core" for the area or "region" by working in a number of places identifiable with major languages in which recognizable "prototypical" similarities among institutions could be found (viz., Gahuku-Gama, Enga, Melpa, Kuma, Chimbu, Mendi, Huli). The category of "region" or pseudo-region was built up by cumulative practice, then, rather than by any logical criteria that could produce a "hard" rather than a fuzzy set. Given this, it was obviously high time to deconstruct the "region" in analytical terms while recognizing that it has today an administrative, political, and interethnic significance that has a life of its own apart from our academic concerns. Hays runs a retrospective analytical eye over contemporary academic usage and finds it wanting; the explanation of how this fuzzy usage has come about could be given only by tracing the development of the discursive practices which have formed usages historically and outside of the academic domain. That problems would eventually emerge is shown even at the heart of the original prototypical enterprise as exemplified in John Barnes's (1962) query concerning African models: we can see from this already that the Huli as described by Glasse seemed not to fit the accounts given for the Enga, Melpa, Chimbu, and Mendi. We can discern the straining for a shared domain of comparative discussion also in Mervyn Meggitt's (1964) early attempt to compare sexual practices and attitudes regarding menstruation across the then "core" of "the Highlands," which included the "Eastern Highlands" cases. Meggitt's ingenuity was severely taxed in the effort to delineate contrasting practices not only between "the Enga" and "the Kuma" but also between the "western" and "eastern" Highlanders (thus "prudes" versus "lechers" as against cases in which elsewhere separated syndromes of attitudes appeared to run in tandem). Along with most of us at the time, Meggitt was engaged in a kind of comparative bricolage, taking data as he could find them within an assumed cultural uni-

verse. Later work was bound to make difficulties for this early enterprise, hence the proliferation of margins, fringes, and (potentially) rival cores that occurred subsequently. Perhaps the only overall framework that in academic terms could have influenced us was the linguistic picture so rapidly delineated by Stephen Wurm (1964) and his colleagues at the Australian National University. This definitely suggested a certain uniformity of origins and perhaps functioned as an image-schema beyond its own limited intentions, for since Boas it had surely been known that language and culture were not isomorphic. Perhaps, though, Hays could have discussed more explicitly the linguistic picture—which also at “the margins” can become fuzzy, as with the ambiguous classification of the Duna language either in the East New Guinea Highlands or the South-West New Guinea Stock.

The ambiguities of boundaries chosen as points of reference can be seen clearly in a latter-day comparative enterprise to which Hays refers only in passing—the debate regarding “big-man” versus “great-man” societies/forms of leadership. The tack chosen in this work has been to obliterate the selective focus on the Highlands by bringing in cases from elsewhere (e.g., Orokaiva, Vanuatu, Rossel Island, Mekeo, Arapesh) while concentrating on a chosen set of structural variables considered significant for the *longue durée*. It is generally a productive strategy, but there is a sense of strain in imposing a single evolutionary model of a transition from great-man to big-man cases; there is the recursion of comparative problems within as well as between categories; and, finally, there is a strain in elevating the category “great-man” into a single analytical type in contrast with “big-man,” which itself had its prototypical start in the “core” of the Highlands and thus never achieved analytical rigor until it was suddenly ranged against its latter-day antitype. Indeed, it is arguable that the beginnings of this process can be discerned in a kind of structural contrast that applies between the “Melpa” and the “Anga,” in spite of the modest variation within the former category and the much broader variation now discerned in the latter. The attempt to create a “hard” category of “great-man society/form of leadership” tends to founder on the fuzzy shores of ethnographic and historical data. None of this implies that the exercise was not worthwhile; it only underlines the hazards of *any* comparative enterprise, no matter what baselines are chosen.

A shift from morphology to process will certainly not enable us to recreate hard sets or units for comparison making. Instead, as Hays suggests, it can become a focus for theorizing in itself. His deft and well-taken suggestions here are much in line with ones pursued in a series of sessions at the Association for Social Anthropology in Oceania yearly conferences from 1988 to 1990. In these sessions we explicitly took the whole island of New Guinea as a unit of reference and deliberately eschewed any concentration on a single imputed region, concerning ourselves rather with concepts of flow and circulation such as were pioneered by James Watson. Later

fieldwork carried out in 1991 by G. Stürzenhofecker and myself in one tiny but morphologically strategic corner of the supposed Highlands has amply demonstrated the value of an antitypologizing approach. In this far-western part of Duna-land, at least, it is necessary to recognize important links and parallels with Ok peoples west of the Strickland and Papuan Plateau peoples to the south as much as with the Huli and Paiela peoples to the east (cf. Strathern and Stürzenhofecker n.d., Biersack n.d.). As linguists have found with regard to dialect chains, we are dealing here with linked chains of cultural processes over time that produce not homogeneity but a mosaic of practices and ideas in flux, and it is this process that we need now to study in detail. In the course of our doing so, regions may disappear and reappear in other guises and with overlapping conformations, but it will become evident that for many reasons (demographic, economic, religious, political) boundaries between them are always permeable and shifting, and typology is always dissolving into history.

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What is the difference between the way we identify “the New Guinea Highlands” or the “Mountain Papuans” and the way Hays elsewhere identifies “the Highlands sacred flute complex” (1988)? Does he make any less of an appeal to characteristics of membership in his definition of what after all is as much an identified and bounded “region” of anthropological discourse as “the New Guinea Highlands”? What, in either case, are the criteria for identifying the “sameness” of the traits of which such regions are composed, whether subsistence regime or origin myth? In which of the two cases is there less of an appeal to boundedness, discreteness, or systematicity? What is the point of criticizing the fuzziness of a concept such as “the New Guinea Highlands” when one does not also criticize the boundedness and referential limits of all the terms in the anthropological repertoire which contribute to such a concept, including, for example, “patrilineality,” “big man,” “initiation,” “flute,” “sacred,” and “myth”? In any case, how does the ambiguity or disagreement concerning the distinguishing features of a term affect the *use* of that term? How can a case be made that full knowledge of such features is a prerequisite for “correct” employment of the term unless one has arbitrarily and tautologously specified beforehand which features one will accept as legitimate?

In short, Hays expresses doubt about the terms of current classifications without at the same time questioning the efficacy of the classificatory process. With the authorities he cites, Hays confuses the notion of family resemblance with the observation that many terms, academic or otherwise, have “fuzzy boundaries.” “Fuzzy boundaries” is a characteristic of a term that has no determinate truth-value. But, as Norman Malcolm reminds us, the determination of truth-value is not what

the notion of family resemblance was intended to address. To say, for example, that the word "chair" applies to a family of cases is to say that there is nothing common to all chairs which justifies the use of the label "chair." It does not mean that there is a family resemblance common to all chairs. "Whenever a word is a family resemblance word there is *no true generalization* that determines its extension. . . . If there is no generalization determining the extension of a predicate there is no 'truth rule' for that predicate" (Malcolm 1978:416).

The whole notion of family resemblance was suggested by Wittgenstein precisely as a counter to the essentialist notions that are so carefully euphemized in the cognitivist writing to which Hays defers. The intent of the contributors to *The Mountain Papuans* was precisely to show that "traits" considered specifically characteristic of the Highlands (e.g., high pig-per-capita ratio, high population density) were also found in societies which otherwise had very little in common with them. It was a polemic deployed against just such categorizations.

Hays calls attempts to delineate bounded regions essentialist. But unlike his own essentialism, the squabbles over Papua New Guinea regions are a by-product rather than a goal of academic debate—a debate which concerns the theoretical and ethnographic status of things like reciprocity, gender, and language. In other words, Hays fails to stipulate the very different ends of discussions of different Papua New Guinea regions. As do Papua New Guineans themselves, anthropologists of this area articulate oppositional contrasts at different levels. Contrasts between the eastern and western highlands are not automatically comparable to those between highlands and fringe highlands, and neither are by definition implicated in a broader highlands-seaboard comparison. Hays's suggestion that the terms of such contrasts ought to retain their significance at different levels and on different scales is surely a most plangent essentialism. It implies that the terms of classification could be stripped of their rhetorical, didactic, and pre-emptive uses within an ongoing conversation among Papua New Guineaists; but these uses, far from being something extra which pragmatically alters or misrepresents their core, semantic value, are the very conditions under which such terms and those to which they are placed in opposition acquire meaning and force. Hays sees the ambiguity and argument surrounding the classification of Papua New Guinea Highlands societies as an undesirable by-product of the "fuzziness" of our terms rather than as the very discursive situation towards which we labour. Therefore his comments do not address or repair the effective terms of debate about Papua New Guinea societies.

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Hays provides us with an insightful critique of the "Highlands" category, calling into question the "essen-

tialist" effort to isolate those traits which separate this region from the surrounding areas. He clearly shows the problems that exist with this effort, which has been the focus of considerable theoretical debate among Melanesianists. More important, he brings to our attention what has been lost as Highlands specialists have struggled to find an agreed-upon set of traits. By searching for what was common internally, we have ignored (1) the heterogeneity of the region's cultures, (2) the influence of colonial forces, and (3) the exchanges between mountain and lowland communities that were a critical part of regional cultural dynamics. Hays asks how we could have ignored these things for so long and goes on to advocate a shift from the essentialist perspective to one that emphasizes cultural process.

I believe that it might also be fruitful to aim a processual light at ourselves. By placing the work of anthropologists in a historical context we might better understand the disciplinary factors that led to the essentialist effort. Given that it has now been 50 years since Reo Fortune's pioneering ethnographic work with the Kainantu peoples, this may now be the time for such reflection. The Highlands gained ethnographic significance after World War II, when anthropology had reached a stage of some academic maturity. It was only just beginning to be interested in social change and still had the tribal world as its primary interest. What more inviting opportunity could there have been than an entirely new region to investigate, and one that had only been "discovered" within the preceding two decades? Theoretically, the structural-functionalism of Radcliffe-Brown came to dominate British anthropology in the 1950s, and what better ethnographic "laboratory" could there have been for the development of a science of society? In the United States in the 1960s, cultural ecology came to have a significance similar to that of British structuralism, and the Highlands created opportunities for the study of tribal environments. Hays should be in a good position to comment on how these historical factors affected the emergence of the "Highlands" category, since he has recently edited a volume of fieldwork recollections by many of the early Highland ethnographers. It may be that the fuzzy "Highlands" category was as much a cultural construct emergent from the historical circumstances of anthropology as it was a set of cultural traits reflective of the peoples of that region.

## Reply

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I am grateful to my colleagues for their thoughtful comments on a paper I wrote with some trepidation. Weiner correctly points out that I myself have written much in the past about "the New Guinea Highlands" and may have been as guilty of "essentialism" as those I criticize.

I am, then, understandably anxious about any suggestion that I have been pursuing, or at least writing about, a chimera for the past two decades. If I had been a commentator rather than the author of this paper, I would have read it carefully indeed.

Such a careful reading would have disclosed, as nearly all of the commentators have realized, that the main focus of my paper is not in fact the highland peoples of New Guinea but "the New Guinea Highlands." That is, it is largely about a descriptive and analytic *construct*, hence my use of quotation marks in the title and throughout the paper. My first objective was to present a critique of how anthropologists (especially) have formulated and used the notion of such a "region." Then, after discussing some of the problems with these usages, I proposed—too starkly, I now recognize—a shift in our focus if we are to be successful in addressing questions that seem refractory to analyses employing units, whether societies or regions, defined on the basis of "traits" they possess.

My critique was prompted in part by a practical issue raised by my impending production of a new edition of a bibliography of "the New Guinea Highlands." In an earlier version (Hays 1976:vii) I noted that "determining the geographical boundaries of 'the Highlands' is . . . difficult since these depend to some extent upon the nature of the problem a researcher is trying to solve." Preferring to err by commission rather than omission, I opted to include "the Mountain Ok," speakers of languages of the Angan family, and the ("Bosavi") peoples of the Great Papuan Plateau as well as the occupants of the central cordillera in Irian Jaya. Inclined to make the same choices again but mindful of the needs of others, I had the issue of scope forced upon me anew: what do anthropologists usually mean by "the New Guinea Highlands"? I therefore surveyed recent, prominent comparative and synthetic studies that significantly deployed the phrase in order to answer my question.

Thus, the object of my analysis was the "Highlands" construct as it has been used in *these* types of works rather than in the ethnographic literature per se. Harrison's charge of unfairness in my purported "objection" that *ethnographers* have been led "to misrepresent these societies as homogeneous, ahistorical, and isolated from the outside world" is, then, misdirected, since representations in the ethnographic literature were not my direct concern. My argument was that highland societies have often been so misrepresented in the *ethnological* literature, but I would further assert that in many ethnographies the "ethnographic present" is alive and well and that homogeneity is certainly implied in common labeling practices whereby descriptive generalizations seem to be offered about whole language groups on the basis of studies of single communities. Moreover, the increasing use of pseudonymous designations portrays communities not only as unconnected to external reference points and events but as unconnectable, except to "insiders" who know the field sites.

Since none of the commentators has either taken issue with my readings and renderings of the works sur-

veyed or cited others to which my criticisms do not apply, I infer that my depiction of the status of "the New Guinea Highlands" as a *descriptive and analytical construct* is probably a fair and accurate one. Put simply, "the New Guinea Highlands" is, in Hayano's words, "a murky anthropological concept"; in mine (emphasis added), "as used in anthropological discourse, [it] exemplifies well what cognitive psychologists would call a 'fuzzy set.'" (Whether highlands societies *necessarily* constitute a fuzzy set is a separate question, to which I shall return below.)

With respect to individual works, the construct is murky or fuzzy because of failure to state what criteria are definitive in assigning ethnographic cases to the category or because of inconsistent or careless application of the criteria chosen. Viewing the works collectively, the murkiness becomes perhaps even more consequential, since, given the varying referential content of the category (cases included or excluded by various writers), the growing list of "traits" attributed to highlands societies is illusory. After some 60 years of ethnographic reports on these societies, we might expect a cumulative picture of "the region" to have emerged, but when different writers refer to different ranges of empirical reality when they talk about "the New Guinea Highlands" we are left, as I said early in the paper, "wondering what we know after all."

Several commentators proffer views on how all of this came to pass, but the question is most usefully addressed at two levels. First, human beings are classifying animals, and we continually seize upon perceived similarities and differences to create worlds filled with categories—of "foods," "people," "societies," "regions," etc. LiPuma's suggestion "that the trope of 'sets' itself should be abandoned" is surely futile. The "epistemological problem" Hayano says I raise but do not address "of how concepts in human language can ever adequately represent 'reality'" is, of course, a hoary one and one that people struggle with in everyday life as much as in scholarly discourse. But life cannot be put on hold until it is solved to everyone's satisfaction. In doing anthropology as in everything else, we create categories, sets, and constructs, including "communities" as well as "regions." While there may be experiential domains, such as "natural kinds" in the biological world, that constrain us in category formation (Atran 1990), when the focus of our interest appears not to come prepackaged in neatly bounded, clearly demarcated chunks, we are prone to create "fuzzy sets." Ogan correctly reads me to be saying that the construction of such sets is "a general human 'information-processing strategy.'" Thus, rather than "assuming that such categories are Bad Things and need to be expunged from anthropological discourse," as Harrison puts it, I regard fuzzy sets as probably unavoidable components of our discourse. I advanced the notion of "the New Guinea Highlands" as a fuzzy set not as an explanatory tool, as Hayano seems to have understood me to be doing, but only as a descriptive one, hoping to draw attention to the pitfalls of reifying what is only a construct that takes its particular



form from the interests and priorities of those who invent it and to caution against "confusing the logic of science with the logic of social formations" (LiPuma).

As for the origins of the particular fuzzy set called "the New Guinea Highlands," Brown, Hayano, Jorgensen, LiPuma, and Strathern point to colonial administrators' actions, and Brown, Jorgensen, Knauff, and Westermarck also cite the role of anthropologists and institutional programs. All of these observations are well-taken, but what is thereby explained is open to debate. Administrators' demarcations of the "Central Highlands," other "districts," and, later, "provinces" have never been based on ethnographic surveys (as anthropological "regions" purportedly are), nor have they ever been presented as reflecting social or cultural boundaries in any simple sense. Probably for this reason, as I pointed out (n. 3), the anthropologists whose works I considered have not constructed "the New Guinea Highlands" following political boundaries. At the same time, one kind of unwitting "collusion" among administrators, missionaries, and anthropologists may have had a great deal to do with the emergent salience of certain highland language groups and their elevation to the "core" as "prototypical Highlanders."

Jorgensen suggests that "the density and scale of Highlands populations" was arguably a key factor in "administrative arrangements marking the Highlands off as a region distinct from the rest of Papua and New Guinea." I pointed out that while writers differ in their assignments of cases, there is apparent consensus that speakers of Enga (or at least Mae Enga), Melpa (around Mount Hagen), Wahgi (Kuma), and Chimbu (especially the Kuman dialect) are charter members of "the New Guinea Highlands," as some of the peoples in the Goroka and Kainantu areas also tend to be. The four language groups named are notably similar in that they are very large by New Guinea standards. Moreover, their constituent populations often manifest somewhat higher densities than do many others. Consequently, I would suggest, they were perceived as both logistically easier to work with and promising greater returns for the efforts of administrators and missionaries. The creation of a "Central Highlands District" and the targeting of Mount Hagen, the Wahgi Valley, the Chimbu Valley, and neighboring Bundi for pioneering missionary work logically followed (see Hays 1992).

When systematic programs of anthropological research in "the Highlands" were launched from Sydney and the Australian National University beginning in 1950 (mentioned by Brown and treated at length in Hays [1992]), both programs were informed by "structural-functional" agendas, as is noted by Westermarck. Given a major emphasis on social organization and anthropologists' long-term preferences for larger populations, it is not surprising that, from 1950 to 1955, A. P. Elkin and S. F. Nadel sent their students and colleagues to conduct pioneering fieldwork among the Enga (Mervyn Meggitt and Ralph Bulmer), the Mendi (D'Arcy Ryan), the Huli (Robert M. Glasse), the Kuma (Marie Reay), and the Kainantu peoples (Catherine and Ronald Berndt, in addition

to James and Virginia Watson, who went there from a different institutional base) and in the Goroka area (Kenneth Read and Richard Salisbury). Soon after, the "Central Highlands" map was filled out with Paula Brown among the Chimbu, Andrew and Marilyn Strathern in the Mount Hagen area, and others. Thus, as Strathern comments, "the ethnographers who were first permitted to work in this area themselves by accident provided the 'cultural core' for the area or 'region' by working in a number of places identifiable with major languages." I would add only that they did so not "by accident" but because of methodological and theoretical biases that are long-standing in our profession. Among the by-products of these choices were that (1) to the extent that "fringe societies" in fact tend to have smaller and more scattered populations, "marginalization" and even "benign neglect" were simultaneous with "centralization," and (2) "traits" such as "big-man leadership" and "ceremonial exchange," which tended to be found among the large, geographically central populations, became reified as "Highlands traits" (Knauff).

Harrison perceives me as suggesting "that we should try to stop thinking in categories altogether." That of course would be, literally, humanly impossible. If he has inferred that I propose we abandon the creation of constructs such as "regions" for comparative purposes, then I was not clear in my paper. I share Knauff's concern that an "emphasis on . . . networks carries the danger of obliterating what is distinctive to particular ethnographic regions," and I keenly appreciate the "creative tension . . . between the use of principled criteria to identify ethnographic regions and the tracing of networks and processes that crosscut them." My true position is that *both* goals are worthwhile. Among the commentators, Strathern and Weiner do appear to reject regional constructs as unfounded or at least unproductive, but when Strathern, citing the "big-man"/"great-man" debate, advocates a focus on "structural variables" regardless of geographical provenience, he still must confront, as all comparativists must do, sampling as well as definitional problems. If Weiner's collection (1988) was "a polemic deployed against just such categorizations" as "the New Guinea Highlands," I find the advancing of another, similar category—"Mountain Papuans"—an odd strategy. In any case, my goal was not to "[down] the disciplinary dragon of Regionalism" (Lederman) but to highlight and urge that we face squarely its inherent challenges: in Harrison's words, "to think more carefully about the terms [Melanesianists] use."

Brown, Hirsch, Jorgensen, and Knauff suggest that there "really is" something distinctive about, and demonstrably common to, the people who inhabit particular, boundable areas. If they want to persuade us of this, the burden is on them as it was on the authors of the works surveyed to address clearly the methodological questions referred to by Hauser-Schäublin: "why we classify, what criteria and methods we use, . . . and . . . whether the system of classification chosen is consistently applied." I agree with Knauff that we need to be



"more rather than less empirically specific in our comparisons and contrasts" but also endorse Hayano's cautionary note that "a simple 'absence of/presence of' coding of behaviors to discern which activities might be characteristic of Highlands, fringe areas, and Lowlands . . . will [or only may?] produce similarities that are superficial and mask differences that are critical." Thus, my main complaint regarding the works surveyed was not that they were intrinsically ill-conceived but that these challenges and strictures were too often not met satisfactorily. Rather than denying the potential value of "morphological" approaches, my point was, with Lederman, that "there are better approaches to 'morphology'" than those I criticized.

As I said in the paper, the reasons for classifying societies into "regions," like the criteria chosen, will vary with the interests and priorities of the classifier. It was not my intent to suggest that comparativists have used the "wrong" criteria in the past or that I know which are the "right" ones. As Hauser-Schäublin recognizes, "what kind of classification is the right one" will depend on "the context and the purpose for which such studies are made." *Contra* Weiner's claim, I would not contend "that the terms of classification could be stripped of their rhetorical, didactic, and pre-emptive uses within an ongoing conversation among Papua New Guinea-ists," but I hope and believe that as ethnographers and comparativists we are engaged in more than a "conversation" among specialists. Yet even in that "conversation," we owe it to the peoples we are characterizing and trying to understand, as well as to each other, to make the definitional criteria we employ and our applications of them explicit, principled, systematic, and accurate.

In my discussion of a shift from "morphology" to "process" in our research, I did not mean to advocate a *replacement* of comparative studies, nor did I intend to suggest that "our research might be *better* focused" with such a shift (Ogan, emphasis added). Rather, I meant only that in the pursuit of some kinds of questions "regions," and perhaps even "communities" and "societies," are inappropriate or misleading units of description and analysis. I cited engaging in trade as one instance of "what people do" that often crosses "regional" boundaries, but this was not intended as the only example worthy of attention, as Hirsch would have me say. Certainly, with respect to trade, Hauser-Schäublin is correct in noting that "this approach is not new for New Guinea," and I would not claim authorship of it, as Harrison implies. Nor would I claim that "focusing on what people do . . . can [ever] *explain* . . . structural relations" or the "politics of identity" (LiPuma, emphasis added). Focusing on "linkages" is a strategy for *discovering* and *depicting* what needs to be explained, including similarities and differences among the units chosen. Thus, as Strathern says, a shift from "morphology" to "process" can "become a focus for theorizing in itself."

As I tried to suggest by alluding to boundary-creating and boundary-maintaining devices deployed by "ethnic groups," if we examine the ways in which people inter-

act with each other across the boundaries implicit in our "morphological" constructs, we can gain insight into "how the relations between groups, cultures, and regions generate structures that permit socially objective categories to emerge" (LiPuma), or, as Lederman puts it, "how any 'region' is made to appear in the first place." According to LiPuma, "Maring culture flows into and overlaps with Kalam culture . . . and Manga culture at the other end of the valley," and Strathern cites "important links and parallels" among the Duna, "Ok peoples," "Papuan Plateau peoples," Huli, and Paiela. These ethnographers' reports exemplify well what I consider to be one of the correctives resulting from our attention to "process": while *for comparative purposes*, constructs such as "Maring," "Kalam," "Duna," etc., are perhaps necessary and useful (if they are carefully and explicitly defined), their reification carries the risk of "[falsifying] all we know of cultural influences and change, relations with neighbors, and intercultural interactions" (Brown). Careful attention to linkages not only checks our impulses toward such reification but also can help us understand how societies and regions, and not just "societies" and "regions," acquire their distinctive character. For both ethnographic and wider purposes, I find LiPuma's analogy an excellent example of the potential reconceptualizations that come from a shift in focus such as that I proposed: "what if ['the New Guinea Highlands'] were more akin to a solar system in which the orbit of any one culture is defined by the gravitational pull and push of all the others?"

Linkages through trade are indeed "an obvious place to start investigation" (Ogan) of "gravitational pull and push," but there are numerous other candidates, some of which have barely, or never, been tapped: warfare and alliance patterns; intergroup marriage (sometimes across language boundaries) that entails not only gene flow but "culture flow"; migration and resettlement, whether government-sponsored or as local coping strategies (Wagner 1971, Waddell 1975); the diffusion and syncretic elaboration of decoration and dance styles (and who knows what else?) that occurs through such catalysts as "the Highlands Show"; and the role of plantation labor experiences or even jail sentences in the fostering or blurring of "ethnic boundaries" through what Lederman calls "indigenous comparative discourses."

As Lederman cautions, studies focused on "process" have their own problems, and we must not suppose that they are simple to conduct. Ogan expresses concern that in a "village-outward" approach "it is not clear . . . what shape(s) our ethnographies might then take." Obviously the very description, let alone understanding and explanation, of some phenomena is not well served by the model followed in typical community studies. Some leads may be suggested by studies such as Finney's (1973, 1987) of entrepreneurship in the Goroka Valley or Sexton's (1986) of the Wok Meri movement in the Daulo Pass area. But, as in comparative studies, we will be forced to deal explicitly and satisfactorily with thorny issues of definition, measures, and delimitation of the scope of inquiry in terms of the cases to be included.

We must be ever mindful of the fact that, like "societies" and "regions," "networks" also are constructs, demanding comparable attention to why and how we create and use them.

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